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Professor Jared S. Moore points out in a note[2] a confusion due to my use of the word *chance* in two distinct senses. (1) When we speak of the two alternatives "design" and "chance," the latter is generally understood to mean either an uncaused result or at least unregulated causation. (2) The notion of *chance variation* in the natural selection theory is somewhat different. When one meets a friend "by chance" we suppose two separate series of events, each of which by itself is causally determined; but the conjunction of the two series at this particular point is not the result of either set of causes. This is, I believe, a legitimate scientific use of the term *chance.* The variations in plants and animals which natural selection works upon are of this sort. Each parental germ-cell is causally determined; the combination of characters in the offspring, though fully "determined," is nevertheless a "chance occurrence" in this sense.

Professor Moore's criticism of my ambiguous use is justified, but I can not agree with the deduction which he draws. Use two words instead of the single word *chance,* or spell Chance with a capital in the first sense, and his objection to admitting that science is the final arbiter in interpreting purpose seems to disappear.

In the judgment of certain other critics my examination of teleology fails to take into account a third alternative cherished by many philosophers, an interpretation which is *neither* entelechial nor mechanistic. Possibly they are correct. Despite many earnest endeavors I have never succeeded in mastering any "philosophical" exposition of teleology from Kant to Bosanquet that has come to my attention. Might it not be worth while for some philosopher to write a "primer" of teleology from this standpoint, for the benefit of the plain every-day scientist who is interested in the problem?

Meanwhile it would seem desirable for mechanistic scientists to avoid using the term *teleology* in their expositions on account of its ambiguity. Till recently I believed it to be a proper equivalent for *the science of purpose.* It appears to be used also to include *harmony* and *trend,* which are quite different concepts and should not be confused with purpose.

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REVIEWS AND ABSTRACTS OF LITERATURE

The Circulation and Sleep: Experimental Investigation Accompanied by an Atlas. JOHN F. SHEPARD. New York: The Macmillan Company. 1914. Pp. ix + 83.

The author of this work measures the blood supply sent to the brain during sleep, in order to determine, if possible, whether sleep is due to anemia, hyperanemia, or to some other cause. He believes that the most accurate method of studying cerebral circulation "is to obtain a record of brain volume together with tracings of general arterial pressure, and in some cases a venus pulse."

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[2] This JOURNAL, Vol. XIII, p. 158.

Two normal male subjects were employed; one of them had been trephined over the right ear, the other on the right side of the forehead. Besides obtaining objective records of the volume of the brain, of the volume of the periphery (hand or foot), as well as curves of breathing, blood pressure, heart rate, transmission time of pulse wave over the body, and of the jugular pulse, the experimenter asked his observers to give a careful introspection whenever they woke during the experiment or at the end of it.

The major part of the monograph is devoted to a detailed explanation of the plethysmographic tracings, which come separately bound and comprise the Atlas.

The main results of the investigation, however, may briefly be summed up as follows: In all positions of the subjects, and whether sleep came rapidly or slowly, there was a marked increase of the volume of the brain as sleep came on. This increase was greatest from fifteen to twenty-five minutes after the subjects had fallen asleep and thereafter it diminished somewhat, but always during sleep the brain volume remained greater than during wake. The absolute values of the volume-changes were between 0.3 cu. cm. and 0.7 cu. cm. Weak stimuli given during sleep caused a fall of the brain volume, but stimuli given during waking caused an increase thereof (p. 37). Waking brings a fall in brain volume, and in general a reverse of the conditions occasioned by sleep. Shepard also noticed that when stimuli were given to the sleeping subject, the responses, as indicated by the tracings from the brain, were perceptibly delayed. Whence he concludes: "There are very definite indications that the circulation change lags behind the mental process" (p. 38).

In view of his findings Shepard declares that the anemia theory of sleep can not be accepted. Neither is sleep due to a separation of dendrites and axons occasioned by the expansion of the cerebral vessels, since there is a similar expansion of these vessels when the subject is stimulated in the waking state. Moreover, the circulation change lags behind the mental process, therefore the relaxation of the brain vessels with sleep and their constriction with waking must be regarded as an effect, not as a cause, of sleep.

Though it is true that sleep may be facilitated by habit, "that one may learn to sleep best in a given environment," just as one learns to attend actively, still this does not always mean that sleep is promoted by withdrawing from external stimuli (p. 77).

What, then, is sleep, and what is it produced by? When sleepiness is analyzed introspectively, says Shepard, it is usually described as "a yielding to the force of gravity," "yielding to heaviness," "becoming lost in rest sensations." These "rest" sensations seem to come from all the muscles of the body; and that is the most noticeable fact about the process of going to sleep. In short, as we fall asleep "we become absorbed in a mass of fatigue sensations" which tend slowly to inhibit other processes, especially motor activity (p. 78). Sleep, then, "consists of a group of sensations of fatigue or rest, . . . sleep itself is a more complete rest," and "the process is a dominance of an organized group of these sensations."

The work is a valuable contribution to the literature of this little understood problem. It shows much ingenuity in technique, and therefore should be very suggestive to physiological psychologists. As the title indicates, it is an investigation that offers more to physiology than to psychology. The only distinctive psychological proposition is the denial of the anemia theory of sleep and the affirmation of the view that sleep is a complex of "fatigue or rest sensations" which inhibit all motor activities and particularly strain sensations.

To speak of sleep, however,—of the bodily state in which all consciousness is lost—as a group of "rest sensations" is equivalent to talking about an unconscious consciousness. A sensation is a mental process, and dreamless sleep is the arrest or the non-existence of all conscious processes; therefore, sleep can not be the "dominance of an organized group of these (rest) sensations." But suppose we assume that Shepard refers to these fatigue sensations as existing only during the process of going to sleep (which is very true), and that they disappear the instant the subject has thoroughly gone to sleep. What is it that keeps the individual in the sleeping state? We should suppose that with the disappearance of these fatigue sensations—these inhibitors—from consciousness, the motor activities which they inhibited a moment ago would pop up again, and the subject would be awake once more.

This subject might have been discussed more fully. It is possible, however, that Shepard will agree with some such view as the following: The fatigue sensations not only inhibit motor activity, but also establish a certain amount of sensorial inertia, *i. e.*, they raise the sensory threshold. The inhibited state of the sensory and motor centers constitutes sleep. Then the continuation of the "rest" or "fatigue" sensations is not necessary to keep these centers in a state of inhibition, for this is maintained by the sensorial inertia that has been established in the first instance. Hence a stimulus of a more intense nature than will ordinarily affect consciousness is required to wake a sleeping individual.

The sensorial inertia view seems to be supported by one of Shepard's own findings. Whenever a stimulus was given to the sleeping agent, there was a considerable delay before the plethysmograph recorded a change in the brain volume. This, the author says, shows that the circulation change lags behind the mental process. But why bring in the term "mental process"? All that the records show is a temporal difference between the application of the stimulus and the circulation change in the brain. May it not be likely that this delay measures the time that it takes a stimulus of the kind given to overcome the sensory inertia?

At this point, however, it will be asked: Suppose sleep is a state that is synonymous with inhibited motor activity, and suppose this state of inhibition is maintained or guarded by the raised sensory threshold, how then does the sleeping subject manage to wake naturally, *i. e.,* of his own accord? It may be answered: first, the stimuli of the dawn—light, sound, etc., are, as a matter of fact, more intense than those that prevail at night; and secondly, there is a radical change in, usually an intensification of, the organic sensations, that overcomes sensorial inertia.

It may also be asked whether the author disproves the view that sleep is produced by and consists of a separation of the dendrites and axons. The evidence he presents against this theory is that there is an increase of brain volume when the individual is stimulated in the waking state, by a drug or what not, and he does not, of course, collapse in sleep. But this argument assumes that there exists a causal relation between the increase of brain volume and the contraction or the separation of the dendrites and axons. No evidence in support of this view is presented.

The author declares that the utility of the relaxation of the brain vessels during sleep "may be that the effective building up of energy-giving substance in the brain requires greater circulation than is demanded by other parts" (p. 78). This is no doubt true. In this connection, however, it might have been worth while to study the brain volume in artificial sleep, *i. e.,* sleep produced by drugs and hypnotism. It is often claimed that artificial sleep is not restful or recuperative. Comparing the blood volume in the brain during the two kinds of sleep might have thrown some desired light on this subject.

It is obvious, however, that too great demands can not be made on observers in experiments that are so taxing as these. The author is to be congratulated on the neatness of the records that comprise the Atlas.

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War and the Ideal of Peace. A Study of those Characteristics of Man that Result in War, and of the Means by which they may be Controlled. HENRY RUTGERS MARSHALL. New York: Duffield and Company. 1915. Pp. 234.

"As long as human nature remains what it is, wars will occur" says many a "practical" man. That human nature will, as a matter of fact, remain what it is he takes for granted. In this assumption he appears to have the support of biology and psychology. For does not war proceed from our instincts, which are the deepest-rooted parts of our nature, and is our nature anything more than a part of the general system of nature with its universal laws? How can the outlook, then, be anything but depressing for those who feel the horror of the present situation?

The main problem of Dr. Marshall's book concerns the validity of the supposedly scientific belief that since war is an expression of our nature and therefore of natural law, it must be expected to recur again and again indefinitely. This is not an isolated question. It involves the whole problem of the relation of ideals to natural law. The author accepts without qualification the assumption that war does express our nature, and that our nature is part and parcel of the natural order that the sciences endeavor to describe in purely mechanistic terms. He denies,